



United States
Department of
Agriculture

Animal and
Plant Health
Inspection Service

RECEIVED SEP 3 7 1982
Federal Building
Hyattsville, MD 20782

Subject: Ivermectin Research

Date: September 2, 1982

To: O. H. Graham
Agricultural Research Service
Mission, Texas

Thank you for the memorandum, "Report on Tests with Ivermectin for Screwworm Control," dated August 13, 1982. The potential benefit of a product like this to the present program and the future barrier at the Isthmus justifies placing a high priority on continued testing.

One of the serious obstacles to forward progress of the program now is the movement of infested animals into free areas. If these animals could be protected, this handicap could be reduced or eliminated. Maybe Ivermectin is the answer. I encourage you to continue the good work that has started.

Joanne P. Garrett

for

Floyd E. Smith
Chief Staff Veterinarian
International Programs Staff
Veterinary Services

*copies were sent to
Spencer
Rung
H.C. Hofmann*

SOUTHWESTERN ENTOMOLOGICAL SOCIETY

P. O. Drawer DG
College Station, Texas 77841

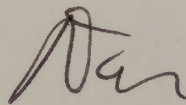
September 6, 1982

O. H. Graham
Screwworm Research
P. O. Box 986
Mission, TX 78572

Dear Dr. Graham:

Your manuscript entitled "Evaluation of the Screwworm Adult Suppression System (SWASS) on the Pacific Coast of Mexico" has been received and you will be notified of its acceptability after review.

Sincerely,



D. L. BULL
Editor

DLB/11w



UNIVERSITY OF ARKANSAS DIVISION OF AGRICULTURE

College of Agriculture and Home Economics • Agricultural Experiment Station

DEPARTMENT OF ENTOMOLOGY

317 Agriculture Building

Fayetteville, Arkansas 72701

(501) 575-2451

RECEIVED SEP 20 1982

September 9, 1982

Dr. Leo La Chance
USDA ARS Lab
State University Station
Fargo, N.D. 58165

Dear Leo:

In the debate stirred up by Richardson et al. claiming many non-interbreeding "strains" of screwworm the specter of parthenogenesis is raised. Little more is said about the likelihood or possibility of parthenogenic screwworms.

I read the excellent article in Volume 7 of the Annual Review of Entomology. It convinced me, among other things, that I had no business delving deeply into the more recent literature.

For some years I did research on biology and control of alfalfa snout beetle, Otiorhynchus (Brachyrhinus) ligustici (L.). It is parthenogenic and is a successful species. Most weevils reproduce sexually. Weevils are well-advanced evolutionarily. Parthenogenesis in alfalfa snout beetle must be a derived and not a primitive character. Eggs are white when laid but those that later hatch turn light tan in color within 2 days of the ca. 7 day incubation period. Some 10% of the eggs remain white and fail to hatch; they are "infertile".

Sterile release of screwworms would not be mutagenic in the sense of "originating" a parthenogenic wild screwworm. If there is or did arise a parthenogenic screwworm, sterile release would strongly select for it.

The great unanswered question is, "What are the chances of there being parthenogenic screwworms?" Can an estimate be made on the basis of current information? These questions probably rank with, "How many fairies can dance on the head of a pin?", but they keep gnawing at me.

Your diagnosis will be appreciated.

Sincerely,

Charles Lincoln of
Lincoln-Eden

nr

cc: W. Eden



United States
Department of
Agriculture

Animal and
Plant Health
Inspection Service

RECEIVED SEP 16 1982

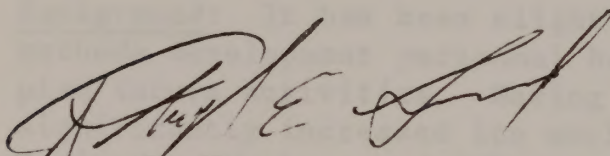
Subject: Joint Research - Methods Development Meeting
in Tuxtla Gutierrez

Date: SEP 10 1982

To: O. H. Graham, Location/Project Leader
Agricultural Research Service, Mission, TX

Thank you for the invitation to attend the screwworm reseach-methods
development meeting held in Tuxtla Gutierrez, September 8, 1982.

Unfortunately previous commitments prevented me from attending.

for 
Norvan L. Meyer
Assistant Deputy Administrator
International Programs
Veterinary Services



United States
Department of
Agriculture

Agricultural
Research
Service

National
Program
Staff

Graham
Beltsville, Maryland
20705

September 13, 1982

SUBJECT: Trip Report--Screwworm Research and Methods Development Planning Session

TO: G. Lambert
Interim Coordinator, AHNPS

Location: Tuxtla Gutierrez, Mexico

Dates: September 8-9, 1982

Participants: Mexico-American Screwworm Commission Co-Directors, ARS Screwworm Project Leaders and Research Scientists, Screwworm Commission Methods Development personnel, and Screwworm Plant Co-Directors and their associates.

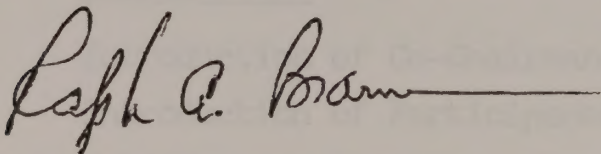
Background: It has been slightly more than 1 year since screwworm research and methods development personnel have met jointly to review accomplishments and plan future activities. During this time, the Screwworm Commission has significantly increased its methods development capabilities at Tuxtla Gutierrez, Mexico, and ARS has not only redirected the research program from Mission, Texas to Tuxtla Gutierrez, Mexico and Fargo, North Dakota, but has also added new scientists. Furthermore, since the last joint meeting of the entire group of research and methods development personnel, ARS has established a Technical Panel for the Study of Reproductive Isolation in Screwworm Populations and APHIS has reactivated the Lincoln-Eden team to review the entire screwworm eradication program including research and methods development. In addition, the assignment of O. H. Graham to Tuxtla Gutierrez as Screwworm Research Project Leader is imminent.

Agenda: The agenda for the meeting may be found as enclosure 1. The ARS scientists presented results of recent research on strain development, attractants and swormlure enhancement, genetics, field ecology, effects of stress on factory-reared flies, rearing and dispersion, and chemical control with Ivermectin®. Of particular note were the facts that the Arriaga strain will replace the currently used V81 strain in about 1 month, that a new strain (Micholima) will be ready to be field tested by Methods Development as soon as they are ready to accept it, and that Swormlure IV has been developed to replace the older formulation Swormlure II. The most pressing needs for additional research include: basic genetics research, nutrition and growth, strain deterioration, chemistry of attraction, population dynamics, supplemental control technologies, and mating behavior.

Methods Development personnel presented results of investigations related to stress in production of screwworm strains, utilization of geletinized media, analysis and field evaluation of the Arriaga strain, and natural screwworm population dynamics.

Both research and methods development personnel agreed in principal to the immediate investigation priorities for the screwworm program. These may be found as enclosure 2.

ARS Research Review. ARS screwworm Project Leaders, Research Scientists, and the NRPL for Insects Affecting Man and Animals met independently to review specifics of recent research accomplishments and to project plans for individual research during the remainder of CY 1982. In addition, the Project Leaders and NRPL developed plans for additional personnel and financial resources necessary to address the recommendations of the Lincoln-Eden Study of the Screwworm Eradication Program. This topic will be the subject of a separate memorandum.



RALPH A. BRAM
National Research Program Leader
Insects Affecting Man & Animals

cc:

T. J. Army
E. L. Kendrick
P. J. Fitzgerald
J. R. Johnston
K. L. Lebsock
✓ O. H. Graham
C. J. Whitten
C. H. Schmidt
L. E. LaChance

A G E N D A

Joint meeting between Agricultural Research Service and Methods
Development - September 8 - 9, 1982

11:00 A.M.

Introduction

Introduction of Co-Chairman - Dr. Bram, Dr. Vargas

Introduction of Participants

Introductory Remarks - Dr. Bushland .

Review of Eden Linclon Report - Dr. Bram

Review of Case situation 1982 - Dr. Pagaza

Review of Production 1982 - Dr. Rios, Dr. Lowe

Review of Research 1982

Fargo

Mission

Tuxtla

Weslaco

Review of Methods Development

Tuxtla

Mexico City

Mission

Programs for 1983

Summary

Research and Methods Development Priorities for
the Screwworm Program

1. Screwworm Commission - ARS joint activities
 - 1.1 Studies of oogenesis
 - 1.2 Sperm motility Strain evaluation and egg production
 - 1.3 Determination of ideal larval weight
 - 1.4 New traps
2. ARS Activities
 - 2.1 Determination of heat requirements
 - 2.2 Development of new strains
 - 2.3 Establishment of a genetic bank (isofemale lines)
 - 2.4 Development of a system to identify when to change strains
 - 2.5 Reevaluation of SWASS
 - 2.6 New attractants
 - 2.7 Ecological studies in the barrier zone
 - 2.8 Male sexual competence
 - 2.9 Screwworm physiology during transport
 - 2.10 Artificial wounds
3. Screwworm Commission Activities
 - 3.1 Adult diet
 - 3.2 Larval diet
 - 3.3 Time of irradiation
 - 3.4 Adequate speed for area dispersion
 - 3.5 Dispersion in canyons



COMISION MEXICO-AMERICANA PARA LA ERRADICACION DEL GUSANO BARRENADOR

LA ENERGIA ATOMICA AL SERVICIO DEL GANADERO

COMTA. 24363

KM. 2 CARRETERA A LA ANGOSTURA

APDO. POSTAL 544

CHIAPA DE CORZO, CHIS.

September 13, 1982

RECEIVED SEP 29 1982

TO: DR. RONALD E. LOWE
SUB-DIRECTOR OF PRODUCTION

SUBJECT: WEEKLY ACTIVITY REPORT
SEPTEMBER 1 - 11, 1982

Field work (Strain evaluation and behavior in the field).

The test in Tapachula has been suspended. The last collections of egg masses and flies of the present phase were made 11 September.

All, 31, egg masses collected 1-11 September hatched. Most collections of flies are yet to be scored.

The test did contribute to technical development in addition to yielding sterility and trapping data. Some items of technical development are listed below:

1. Areas (if similar to the Tapachula-Area) 30 x 30 Km (perhaps smaller) can be used to test strains in the field.
2. Pupae, in bulk, can be stored or transported by common carrier without air exchange systems for up to 17 hours (perhaps longer) and still yield viable adults.
3. Daily, one person using 1 pick-up can distribute, where roads are adequate, pupae at the rate of 2000 per Km² (ca. 5125 mi²), or multiples or fractions thereof, without the cost of boxing pupae then feeding, storing, transporting, and distributing flies. Therefore, in one week 14,000 Km² could thus be treated.
4. Relatively large numbers of marked flies, again with the use of cheap transportation and no expense related to boxed flies, can be placed in the field at fixed points so that migration and behavior can be studied.

...#

Revised 10-15-55

September 15, 1955

Mr. J. J. J. J.

Subject: MEXICO-AMERICAN COMMISSION FOR THE TEACHING OF ENGLISH

Dear Sir: (Enclosed for you are two copies of the report.)

The first is a report on the work of the Commission during the year 1954-1955. The second is a report on the work of the Commission during the year 1955-1956.

Very truly yours,
J. J. J. J.

Enclosed for you are two copies of the report. The first is a report on the work of the Commission during the year 1954-1955. The second is a report on the work of the Commission during the year 1955-1956.

1. From the studies in the English-Spanish field, the Commission has found that the English-Spanish field is a field of great importance and interest.

2. In the field of English-Spanish, the Commission has found that the English-Spanish field is a field of great importance and interest.

3. In the field of English-Spanish, the Commission has found that the English-Spanish field is a field of great importance and interest.

4. In the field of English-Spanish, the Commission has found that the English-Spanish field is a field of great importance and interest.

The test would not have been possible had it not been for detailed planning by Dr. R. J. Brenner (ARS), encouragement and fiscal support rendered by Dr. O. H. Graham (ARS), administrative support by Dr. J. P. Spencer (ARS), and a recognition of the potential value of the test by Drs. R. E. Lowe and H. C. Hofmann (APHIS).

S i n c e r e l y

D. E. Hopkins

D. E. HOPKINS
BIOTECHNICIAN

c.c. Dr. R. J. Brenner
Dr. H. C. Hofmann
Dr. J. P. Spencer
Dr. O. H. Graham
Dr. Ralph Bram
Dr. R. C. Bushland
Mr. C. M. MacVean

*svrg



United States
Department of
Agriculture

Agricultural
Research
Service

Southern Region
Oklahoma-Texas Area

Dr. Graham
Screwworm Research
P. O. Box 986
Mission, TX 78572

September 15, 1982

Dr. Morton Beroza
821 Malta Lane
Silver Spring, Maryland 20901

Dear Morton,

Thank you for the information you supplied during our recent telephone conversation. As promised, I am enclosing some information about the chemical composition of Swormlure-2 (used in the eradication program for about 4 years), Swormlure-4 (currently being used), and SWASS (Screwworm Adult Suppression System) pellets (attractant-bait-toxicant combination). The pellets, which resemble pelleted cattle feed, are distributed by airplane at the rate of one pound per square mile, 2X per week for 3 weeks (sometimes referred to as a "standard" SWASS treatment).

In the field the swormlure volatilizes rapidly and under ordinary conditions SWASS pellets lose most of their attractancy within 48 hrs. SWASS was first tested in Texas and northern Mexico and, in some of the tests, populations of adult screwworms were reduced about 75% by a "standard" treatment. Under some of the climatic conditions encountered in central and southern Mexico swormlure-2 appears to be less attractive than it was in Texas and the swormlure-4 formula was developed in an attempt to enhance attractiveness in the humid tropics.

In regions where "failures" have been observed there is often much less air movement than in South Texas. This has led to speculation that the odor plume from a SWASS pellet in the field is much smaller (shorter) in the tropics and that it might be advantageous to formulate a smaller pellet or wafer and distribute more pellets per square mile. If the present formulation is used to make a small pellet, say < 1 g, instead of a 3.3 g pellet, most of the swormlure is volatilized within a few hours. This has led to discussion of the possible use of some type of controlled release technology to give the small pellet a life in the field of at least 2 to 3 days. Another possible goal would be the formulation of a larger, water repellent pellet with a life of 1 to 2 weeks or even longer.

If you have any suggestions for research approaches that might be useful we would appreciate receiving them. Please call me collect (512/585-2783) if it would be helpful. With best personal regards, I am

Sincerely,

Hugh
O. H. GRAHAM
Location/Project Leader

cc: Hoffman
Brown Plimmer
Mackley Bram

Enclosures

Composition of Swormlure-2

Components	Percent W/V
<u>sec</u> -butyl alcohol	10.5
<u>iso</u> -butyl alcohol	10.3
dimethyl disulfide	12.0
acetic acid	13.7
butyric acid	12.5
valeric acid	12.2
phenol	12.2
p-cresol	11.4
benzoic acid	2.6
indole	2.6



United States
Department of
Agriculture

Science and
Education
Administration

Agricultural Research
North Central Region
Metabolism and
Radiation Research
Laboratory

P.O. Box 5674
State University Station
Fargo, North Dakota
58105

September 17, 1982

Dr. Charles Lincoln
University of Arkansas Division
of Agriculture
Department of Entomology
317 Agriculture Building
Fayetteville, Arkansas 72701

Dear Charley:

Thank you for your letter of September 9th. It was a great pleasure hearing from you. Your question about the possibility of parthenogenesis in screwworms sent me to the library and forced me to do some "contemplating" on the subject. I haven't done enough of either and am not done yet, however, I will give you my thoughts on the subject. Before reading further I must warn you that my answer will not be definite and I may wander around a bit before I finish.

Parthenogenesis in insects is very widespread. In some orders it is the natural state of affairs, in others it is rare. The frequency of parthenogenesis in the Diptera is not high, but high enough to make me feel uneasy. Several examples are given in Suomalainen's review (Ann. Rev. of Ent. 7:1962) which you have read. In addition, it is found in some strains of Culex pipiens and fairly often in Drosophila species (Attachment #1). However, the most important question is not how many times an investigator can FORCE a parthenogenetic strain out of a sexually reproducing species in the laboratory but how often it will arise spontaneously, become established, and become the "preferred mode" of reproduction.

I believe that Richardson's speculations on parthenogenesis in screwworms stem largely from some recent, and very interesting, work on Drosophila mercatorum by Templeton (Att. #2). Templeton's work is complex and elegant and I won't go into the details. Essentially, he forced parthenogenetic strains out of a previously sexually reproducing species. He studied these lines extensively but the following points are important:

1. The probability of an unfertilized egg (female) giving rise to a parthenogenetic strain is rather low, $P = 10^{-5}$ to 10^{-6} .
2. The reproductive transition is a selective one that results in the survival of only rarely occurring genomes that are viable when totally homozygous and coadapted to parthenogenesis.
3. In order to make the transition the strain must undergo a tremendous selective bottleneck: a) the elimination of recessive lethals or lethal complexes, and b) the favoring of genomes coadapted to parthenogenetic reproduction and its genetic consequences.

Thus, if we want to estimate the probability of a parthenogenetic strain of screwworms arising we should consider the probabilities:

1. A female (or an unfertilized egg) reproducing parthenogenetically, $P = 10^{-6}$.

2. The resulting progeny will be viable when totally homozygous and coadapted to parthenogenesis, $P = ?$.

3. When the above two events occur simultaneously, the resulting strain will "prefer" to reproduce parthenogenetically rather than sexually, $P = ?$. Thus, the probability of a parthenogenetic strain of screwworms arising is probably in the order of $P = 10^{-6} \times ? \times ?$. I admit that this is not a very good mathematical expression, but the best that I can do at the moment.

There is another point I wish to make. Almost everyone agrees that if a parthenogenetic strain of screwworms did arise, sterile releases would strongly select for it.

This statement has been made often enough so that it is commonly accepted. However, I have some reservations. I am not sure that sterile releases would select for parthenogenetic reproduction for the following reasons. Let us assume that a parthenogenetic strain does arise. A single unmated female lays 250 eggs that hatch and produce about 100 daughters that are capable of parthenogenetic reproduction. I repeat "capable" because parthenogenesis could be either facultative or obligatory. If it is facultative then these females could mate with either a native male or a released male. If they mate with a released male then the strain becomes extinct. If they mate with a native male then they produce either bisexual progeny or unisexual progeny (parthenogenetically). Thus, it seems to me that for sterile releases to strongly select for parthenogenesis two things must happen simultaneously: 1) the parthenogenetic strain must arise, and 2) it must immediately be reproductively isolated from native and released males ($P = ?$). Under these conditions then the released males would mate with sexually reproducing females and reduce the sexual population and the parthenogenetic strain would increase in frequency, and the program would be in serious trouble.

You also ask, "Can an estimate be made on the basis of current information?" In addition to the above we can consider the following:

1. Parthenogenetic strains have not arisen (or did not exist) in the in Curacao, the Southeastern U. S., the entire Southwestern U. S., Baja California, or Northern Mexico. However, these populations could perhaps be considered "introduced" or "fringe" populations with less genetic variability. Templeton shows that in populations with considerable genetic heterogeneity the probability of parthenogenesis arising is greater. As the program encounters year-round populations in southern Mexico where screwworms have existed for centuries, then genetic variability is likely to be much greater, hence, the probability increases.

2. Dr. Whitten feels that screwworm researchers have collected a fairly substantial number of egg masses in the past and reared the progeny. In virtually all cases where 5 or more progeny were reared, both sexes were found (not parthenogenetic). However, these samples are certainly not adequate to firmly rule out the possibility.

3. Even if we collected 10,000 egg masses from virgin females and found that none of them hatched we might be sampling from the wrong population. If we found that 0.0001% hatched and produced viable adults then we would have an interesting strain to study. Would it be reproductively isolated? Would parthenogenesis be facultative or obligatory?

4. Four years ago, Dr. E. F. Knipling had this to say on the subject: "It has puzzled me why the vast majority of insect species continue to reproduce bisexually when there would seem to be such a great advantage to parthenogenetic reproduction because of density dependent suppression forces. Over a period of millions of years hundreds of thousands of insect species must have experienced numerous catastrophies that reduced numbers in isolated areas to such low levels that mate finding became difficult. Also, in migration or drift with wind currents, single females of many species must have been completely isolated many, many times. Such situations should be conducive to the development of parthenogenetic strains. However, since such species seem to be rather few in number in comparison with bisexual species, I have felt that there must be some inherent disadvantages to parthenogenecity and that in the end parthenogenetic strains are not likely to evolve."

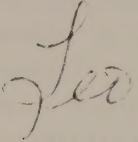
From these statements you may conclude that my position is that parthenogenesis is not an important problem for the screwworm program. I continue to feel a bit uneasy about the subject but we do have a lot of more important problems to resolve this year and next.

It has often been said that "Any species of insect will eventually develop resistance to any control method." I believe this is true. How resistance to sterile releases will eventually evolve is what we are all trying to guess. However, development of parthenogenetic strains may not be the mechanism. In any case, it seems important not to rely on a single suppressive method for extended periods. For this reason, I feel that development of alternative suppressive measures, such as SWASS and other attractants, and increasing our knowledge of the genetics and biology of natural populations is critical to the continued success of the screwworm program.

I hope my wandering thoughts are of some use to you. This letter reminds me of the old saying, "You ask that guy the time and he builds you a Swiss watch". I would really like to hear from you again concerning this or any other genetic question. I only hope the next question will be simpler.

In closing, I want to thank you and Bill Eden for giving me the opportunity to serve with the consultant team on the 1982 Lincoln-Eden Review. It was a very educational and most rewarding experience. I have read the final report and congratulate you both on the fine editing job. It turned out much better than I expected when I left Memphis.

Best always,



L. E. LaChance
Research Geneticist -
National Technical Advisor
for Insect Genetics

cc: Dr. Eden

present, have arisen in insects more than four or five times. Evidently generative parthenogenesis is, at least in most cases, of very old origin [for details, see Whiting (112)].

In many insects, the unfertilized eggs of many bisexual species begin to develop, although the development as a rule sooner or later comes to a standstill (rudimentary parthenogenesis). Sometimes such unfertilized eggs develop occasionally through parthenogenesis quite far, even to adult stage in cases of accidental parthenogenesis or tytoparthenogenesis. Many authors have suggested that tytoparthenogenesis may be regarded as the primitive type from which normal thelytoky has developed. Of the animals whose eggs have been artificially induced to develop parthenogenetically, it has been established that certain strains show a definitely stronger tendency toward parthenogenesis than others. This, among other things, has been established by Astaurov (1) in the moth of the silkworm, *Bombyx mori* (Linnaeus); it depends on certain genes. It is definite that in animals which normally develop parthenogenetically parthenogenesis also depends on certain genes and gene combinations.

The origin of parthenogenesis is, so far, obscure in its details. However, recent investigations on the flies of the family Drosophilidae and on bagworm moths (*Solenobia*) have given new clues.

Stalker (87) discovered in a survey of 28 members of the family Drosophilidae a low rate of parthenogenesis in 23 species. Of these, only three: *Drosophila parthenogenetica*, *D. polymorpha* Dobzhansky & Pavan, and *D. affinis* Sturtevant produced adult progeny. In the two last mentioned, however, the frequency of parthenogenetically developing adults was very low, the rate of parthenogenesis being 2/37,628 and 1/19,059. In *D. parthenogenetica* about 1.5 per cent of the eggs laid by virgin females develop into adult offspring. Later, Carson (12) has found a similar phenomenon in *D. robusta* Sturtevant, the rate of parthenogenesis in this species being 0.93 adult females per million eggs. Further Carson, Wheeler & Heed (13) and Murdy & Carson (46) have found in *D. mangabeirai* a wholly parthenogenetic strain which displays about 60 per cent hatchability of unfertilized eggs; of these about 80 per cent survive to the adult stage. Further, Stalker (87, 88) has found that in *D. parthenogenetica* and *D. polymorpha* continued selection for parthenogenesis results in a gradual increase in the rate of parthenogenesis over many generations and, hence, is probably based on a polygenic system.

Stalker (87) and Sprackling (86) showed that parthenogenesis in *D. parthenogenetica* is automictic and the diploidy is reconstituted by a fusion of two haploid polar nuclei following normal meiosis. Stalker observed that terminal and central fusions occur with equal frequency, and in addition the incidence of triple fusion is high; 23 per cent of parthenogenetically developed flies were found to be triploid. Normal meiosis and automictic fusion of two haploid polar nuclei have been described also for the parthenogenetic strain of *D. mangabeirai* in which meiotic spindle

THE UNIT OF SELECTION IN *DROSOPHILA MERCATORUM*. II. GENETIC REVOLUTION AND THE ORIGIN OF COADAPTED GENOMES IN PARTHENOGENETIC STRAINS*

ALAN R. TEMPLETON

*Department of Biology, Washington University,
St. Louis, Missouri 63130*

Manuscript received August 15, 1978
Revised copy received December 4, 1978

ABSTRACT

Drosophila mercatorum is a sexual species that can reproduce parthenogenetically. Previous studies revealed that parthenogenetic strains had "coadapted genomes" with high fitness under parthenogenesis and total homozygosity due to nonadditive and nonmultiplicative fitness interactions between chromosomal segments scattered throughout the genome. To study the evolutionary origins of such coadapted genomes, females from sexual matings in nature were isolated as virgins and challenged to reproduce parthenogenetically. Fitness studies were performed on genomes derived from these sexual females and upon their successful parthenogenetic progeny. By straddling the reproductive transition from sex to parthenogenesis, these fitness studies demonstrated that coadapted genomes arise immediately, apparently due to an intense selective bottleneck accompanying the reproductive transition, and are not due to the slow accumulation of epistatic complexes *via* mutation after parthenogenesis has already been established. The reproductive transition may also serve as an experimental model of the "genetic revolution" theory of speciation because the transition involves (1) the ultimate founder effect (one genome), (2) maximal genetic drift and fixation, (3) a drastic change in genetic environment characterized by total homozygosity, and (4) an intense selective bottleneck that interacts with the change in genetic environment and the need to adapt to a laboratory environment and a novel system of reproduction. Thus, all the elements theorized to underlie genetic revolution are present, albeit in extreme form. This study indicates that genetic revolutions are real phenomena that can quickly alter morphology, development, life history parameters and behavior. Indeed, the alterations can be so drastic that a new "species" evolves, complete with pre- and post-mating isolating mechanisms. However, isozyme loci do not appear to be the target of this genetic revolution, but rather loci regulating fundamental developmental processes. However, isozyme loci may be useful in predicting the *a priori* chance of a successful revolution since they can indicate how the population structure of the parent population influences levels of individual heterozygosity, the prime source of the genetic variability in the founder population that must pass through the selective bottleneck.

D*ROSOPHILA mercatorum* is normally a sexually reproducing fly that can, in the laboratory, reproduce parthenogenetically by an automictic mech-

* Supported by National Science Foundation Grants BMS 74-17453, DEB 76-16985 and DEB 78-10455.

att #2

THE PARTHENOGENETIC CAPACITIES AND GENETIC STRUCTURES OF SYMPATRIC POPULATIONS OF *DROSOPHILA MERCATORUM* AND *DROSOPHILA HYDEI*

ALAN R. TEMPLETON

Department of Biology, Washington University,
St. Louis, Missouri 63130

Manuscript received August 4, 1978

Revised copy received December 5, 1978

ABSTRACT

Drosophila mercatorum is a sexual species that can reproduce parthenogenetically in the laboratory. A previous study showed that a natural population of *D. mercatorum* inhabiting the Kamuela garbage dump on the Island of Hawaii could produce both viable parthenogenetic adults and self-sustaining parthenogenetic lines. The present study deals with a second screen for parthenogenesis and an isozyme survey performed on natural populations of *D. mercatorum* and *D. hydei* caught in patches of *Opuntia tuna* about 10 kilometers from Kamuela. Both cactus-patch species produced viable parthenogenetic adults, but only *D. mercatorum* produced parthenogenetic females themselves capable of parthenogenesis. Moreover, *D. mercatorum* produced several "hot" lines characterized by high parthenogenetic rates, while all lines of *D. hydei* had a homogenous low rate. The parthenogenetic capacity of the cactus-patch *D. mercatorum* was lower than that of the garbage-dump *D. mercatorum*. Moreover, both the cactus-patch *D. mercatorum* and *D. hydei* had lower levels of polymorphism (26% and 22%, respectively) than the garbage-dump *D. mercatorum* (44%), and both cactus-patch populations had heterozygote deficiencies with respect to Hardy-Weinberg equilibrium, unlike the garbage-dump population. Consequently, these data do not support the idea that decreased levels of heterozygosity in a sexual population increase the chance that sexual females will produce totally homozygous, parthenogenetic progeny.

STALKER (1954) surveyed 28 species of *Drosophilidae* for their ability to reproduce parthenogenetically and discovered at least some capacity for parthenogenetic development in 23 of them. However, only three actually produced adult parthenogenetic progeny (*D. parthenogenetica*, *D. polymorpha* and *D. affinis*). Since STALKER's pioneering work, parthenogenetic adults have been discovered in five other normally sexual species of *Drosophila* (*D. robusta*; CARSON 1961; *D. mercatorum*; CARSON 1967; *D. ananassae* and *D. pallidosa*; HUTCH 1973; *D. paulistorum*, EHRMAN, personal communication). Of these species, *D. mercatorum* is of particular interest because its parthenogenetic stocks are easily reared in the laboratory (CARSON 1967). Moreover, TEMPLETON, CARSON and SING (1976) showed that new parthenogenetic strains of *D. mer-*

IOWA STATE UNIVERSITY

of Science and Technology

AMES, IOWA 50011

Department of Entomology

September 22, 1982

Dr. O. H. Graham, ARS, USDA
Screwworm Research
P.O. Box 986
Mission, TX 78572

Dear Hugh:

One of our students with an aptitude for animal and field work has expressed very strong interest in joining the screwworm program. He is Bob Dickeson and he will graduate this semester.

I take the liberty of inviting your attention to Mr. Dickeson's candidacy and qualifications. I'm certain that Bob would be a happy and productive employee in Mexico and therefore worthy of consideration.

With best wishes.

Sincerely,

E. S. Krafur

E. S. Krafur
Associate Professor

ESK:mc

*See application for Employment file
for SF 171 & supporting papers*



United States
Department of
Agriculture

Agricultural
Research
Service

Southern Region
Oklahoma-Texas Area

Dr. Graham
Screwworm Research
P. O. Box 986
Mission, TX 78572

September 20, 1982

SUBJECT: Report of the Technical Panel for the Study of Reproductive Isolation
in Screwworm Populations

TO: Jack A. Seawright, RL
IAMARL, Gainesville, Florida

Because of travel and other interruptions I only recently read my copy of the exchange of correspondence between you and Dr. Bram on this subject. Like Dr. Bram I very much appreciate your support of the screwworm research program and your remarks about the need for increased funding. You may know that the Lincoln-Eden team also recognized this need and recently recommended that the funds for screwworm research be more than doubled.

We of course regret that Dr. McInnis decided to transfer to Hawaii as he, with almost 4 years of experience in screwworm research, could have helped us considerably had he chosen to go to Tuxtla Gutierrez. You may know that we have approval to replace him promptly and expect an announcement for a Research Geneticist to be published in October. If you know qualified persons who would like to work on screwworm genetics in Tuxtla, please call their attention to this announcement when it appears.

Fortunately, we were able to hire Dr. Robert L. Mangan in February of this year. Bob is an entomologist with extensive training in evolutionary biology as well as field experience in Mexico and I am confident that he will substantially improve our understanding of screwworm genetics, especially in tropical Mexico.

You have helped the screwworm research group in many ways for several years and we very much appreciate your willingness to continue to do so. Your incisive reviews of screwworm publications have been especially helpful. We are happy to know that we will be able to continue to call you on for advice and help.

Sincerely,

O. H. GRAHAM
Location/Project Leader

cc: D. E. Weidhaas
L. E. LaChance
C. J. Whitten
J. R. Johnston
R. A. Bram

RECEIVED SEP 27 1982



United States
Department of
Agriculture

Agricultural
Research
Service

Southern Region
Insects Affecting
Man and Animals
Research Laboratory

1600 SW 23rd Drive
P.O. Box 14565
Gainesville, Florida
32604

September 23, 1982

SUBJECT: Research Geneticist for Screwworm Program

TO: O. H. Graham
Screwworm Research
P. O. Box 986
Mission, Texas 78572

Thank you for your kind remarks in your memo of September 20. I have two scientists that I strongly recommend for the vacancy in Mexico. Their names and phone numbers are:

Dr. Satish Bhalla, 202-468-2500 - extension 459
202-948-0376 - (home)

Dr. William Steiner, 217-333-6897

Dr. Bhalla took his Ph.D. (Genetics of mosquitoes) at Notre Dame University under the tutelage of Dr. G. B. Craig, who can be consulted about Dr. Bhalla's qualifications. Dr. Bhalla has lived in Brazil for several years and is competent in Latin American languages. Currently, he works for a consulting firm in Washington, D.C. Overall, I rate him as a very good prospect for the work in Mexico because of his native intelligence, training, and experience; it shall be very difficult to find a better qualified person.

Dr. Steiner is on the faculty at the University of Illinois. He has worked, during periods of 3-4 months, in various parts of South America on projects dealing with population genetics of mosquitoes. He has excellent training and is an experienced scientist, but he may be reluctant to consider a job in Tuxtla because of his school-age children. However, his present position is funded by a grant, so he may jump on the opportunity of a permanent position.

I think highly of both men, and I am sure that either would be able to make a significant contribution in fulfilling the mission of the screwworm research unit.

JACK A. SEAWRIGHT
Research Leader

cc:
R. Bram
L. LaChance

September 23, 1982

SUBJECT: Formulation of Swormlure

TO: The Files

On September 21 I received a call from Dr. Jim Mackley, ARS, Tuxtla Gutierrez, Mexico, to discuss the formulation of a screwworm attractant. Under the conditions prevailing in Central Mexico, Swormlure-2 formulations and SWASS formulations lose their attractancy rapidly. The modified formulation, Swormlure 4, was being used and, in this modification, quantities of the more volatile components were increased.

The pellets are scattered by air and it is considered that a smaller pellet than those currently used might be advantageous. The release rate should be such that activity is maintained for 3-4 days.

The question of suitable controlled release techniques was raised. We conducted some research on this topic several years ago and the Laboratory evaluated several controlled release devices before the wax formulation of swormlure was adopted. However, some controlled release techniques might be adapted to the solution of this problem and I will attempt to identify candidates for study.

The attached copy of a memorandum from Dr. O. H. Graham indicates that the formulation problem has been brought to the attention of the Program Staff.

/S/ Jack R. Plimmer

JACK R. PLIMMER, Chief
Organic Chemical Synthesis Laboratory
Agricultural Environmental Quality Institute

cc:

B. A. Leonhardt

W. Klassen

J. Mackley

R. Bram

H. Brown

~~O.~~ H. Graham



COMISION MEXICO-AMERICANA PARA LA ERRADICACION DEL GUSANO BARRENADOR

LA ENERGIA ATOMICA AL SERVICIO DEL GANADERO

COMTA. 24363

KM. 2 CARRETERA A LA ANGOSTURA
CHIAPA DE CORZO, CHIS.

APDO. POSTAL 544

23 de septiembre de 1982

September 23, 1982

PARA: DR. MARIO A. LICON
SUB-DIRECTOR GENERAL

TO: DR. JAMES E. NOVY
GENERAL SUB-DIRECTOR

ASUNTO: CONTROL DE MOSCAS CASERAS

SUBJECT: HOUSEFLY CONTROL

Al enviar esta carta estoy exponiendo un punto de vista que difiere al de otras personas, pero es lo que realmente pienso por conocer los factores dominantes, y sería un acto de negligencia no hacer los siguientes comentarios:

• By sending this letter I am presenting a view that does not coincide with that of all others concerned, but I feel that I know the pertinent dominant facts; I would be remiss if I did not make the comments that follow:

Es notorio que la contaminación de la mosca casera en el interior de la planta ha estado bajo control durante un largo período de producción normal. (Anteriormente se había controlado durante períodos cortos, cuando se iban a efectuar los cambios de cepas). El factor principal que permite contar con un control permanente es un sistema por medio del cual se evita que las moscas escapen fácilmente del área de pupación.

It is apparent that the housefly contamination within the plant has now been controlled for a prolonged period of regular production. (Before the contamination had been controlled for short periods when strain changes were being made). The primary factor that promotes the lasting control is a system that prevents the easy exit of flies from the pupal-storage-facility.

La razón primordial para ese sistema de control es Raquel Courtois.

The No. 1 reason for the system that controls is Raquel Courtois.

Raquel formuló el plan básico para el sistema y se mobilizó para promover su aprovechamiento; ese hecho es inequívoco y está bien documentado, como se puede comprobar en las copias que adjunto. (Quizá alguien más tuvo las ideas básicas, pero fue Raquel quien hizo todo lo necesario para que se realizara).

Raquel devised the basic plan for the system and acted to promote its utilization; that fact is unequivocal and is well documented --enclosures (7) are for documentation. (Perhaps other had also conceived the basic ideas but Raquel did what was necessary to see his concepts materialize).

Reconocer y recompensar oficialmente a Raquel por su valiosa contribución sería únicamente un acto justo, pero mas que justicia, sería bueno que se recompensara lo que se recibe, de esta manera las

To officially acknowledge and reward Raquel for his invaluable contribution would only be just; however, more than justice would be served if what were due were delivered --the goals of

....#

metas de la Comisión estarían mas próximas; esas metas estarían mas cercanas porque de esa forma la Comisión fomentaría el espíritu creativo y progresista de otras personas.

the Commission would be nearer; those goals would be nearer because the Commission would thereby encourage the creative and progressive spirit in other.

A t e n t a m e n t e

S i n c e r e l y

Donald E. Hopkins

DONALD E. HOPKINS
SUPVRY. BIOTECHNICIAN

c.c. Dr. Eduardo Rios Salas
c.c. Dr. Ronald E. Lowe
c.c. Dr. Martinez y Tapia
c.c. Sr. Charles McVein
c.c. Dr. Chris H. Hofmann
c.c. Dr. Moisés Vargas T.
c.c. Dr. O. H. Graham
c.c. Sr. Raquel Courtois

*svrg

14 de Agosto de 1981.

A: DR. ARTURO MARTINEZ Y TAPIA.

DR. RONALD E. LOW.

JEFES DEL DPTO. DE INVEST. Y DES. EXPERIMENTAL.

ASUNTO: "ANTEPROYECTO DE UN POSIBLE SISTEMA PARA CONTROLAR LA CONTAMINACION CON Musca domestica, EN EL INTERIOR DE LA PLANTA".

Es ya un problema bastante conocido y al cual se le ha tratado de dar alguna solución, la contaminación con M. domestica en el interior de la planta; no obstante al no tener la colaboración por parte del personal obrero, prácticamente el problema continúa y se torna potencialmente peligroso. Es por ello que se requiere de un plan en el cual no se requiera de la colaboración del personal antes mencionado, por lo que pongo a su consideración lo siguiente:

Sabemos que la M. domestica emerge más o menos 12-24 hrs. antes de que la pupa de C. hominivorax sea irradiada, por lo tanto nace en el cuarto denominado de 5 $\frac{1}{2}$ Dias. Se han colocado barreras con puertas y cortinas para aislar esta zona de la de Desarrollo Larvario (Piso), pero los obreros y el personal de Mantenimiento prácticamente destruyen dichas barreras debido a que obstruyen el paso. En virtud de la imposibilidad de obtener la colaboración del personal, es necesario aislar de alguna manera el foco de emergencia de la M. domestica. es decir, el cuarto de 5 $\frac{1}{2}$ Dias. Este aislamiento sería muy fácil de hacer cerrando ambas puertas (acceso y salida de anaqueles), del cuarto de 5 $\frac{1}{2}$ Dias, y manejando la pupa de C. hominivorax por medio de dos ductos de acero inoxidable; no obstante el problema es la recirculación de los anaqueles en el interior del cuarto. No es posible hacerlo por la falta de monorriel y espacio. Entonces, esto puede resolverse colocando un tramo de monorriel (14 mts. aproximadamente) que una los monorrieles de acceso y salida por la parte exterior, y aislando dicho monorriel con un túnel de mallá y a través de dicho túnel llevar a cabo la recirculación de anaqueles. En ambos extremos del túnel se colocarían los ductos de acceso y salida de la pupa de C. hominivorax evitnado así el escape de M. domestica fuera del área de 5 $\frac{1}{2}$ Dias. Dicha mosca puede destruirse fácilmente dentro de este cuarto con cualquiera de los métodos convencionales.

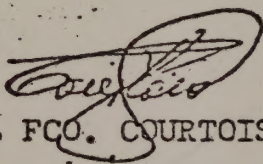
La ventaja de éste sistema es que no obstruye el paso del gruezo del personal por lo que no existe la posibilidad de su destrucción. Se anexa un diseño muy simplificado del área en cuestión, con las dimensiones aproximadas.

Para mayor y mejor entendimiento acerca de este anteproyecto, puedo ponerme en contacto con Ingeniería y Mantenimiento para que ellos delimiten las posibilidades del funcionamiento de este mismo.

Comunicoa Uds. que el Sr. DONALD E. HOPKINS considera dicho proyecto con muchas posibilidades de éxito, mismo que está tomando parte activa en éste.

Esperando su consideración para con esta sencilla sugerencia con el deseo de contrarrestar este molesto insecto.

ATENTAMENTE:

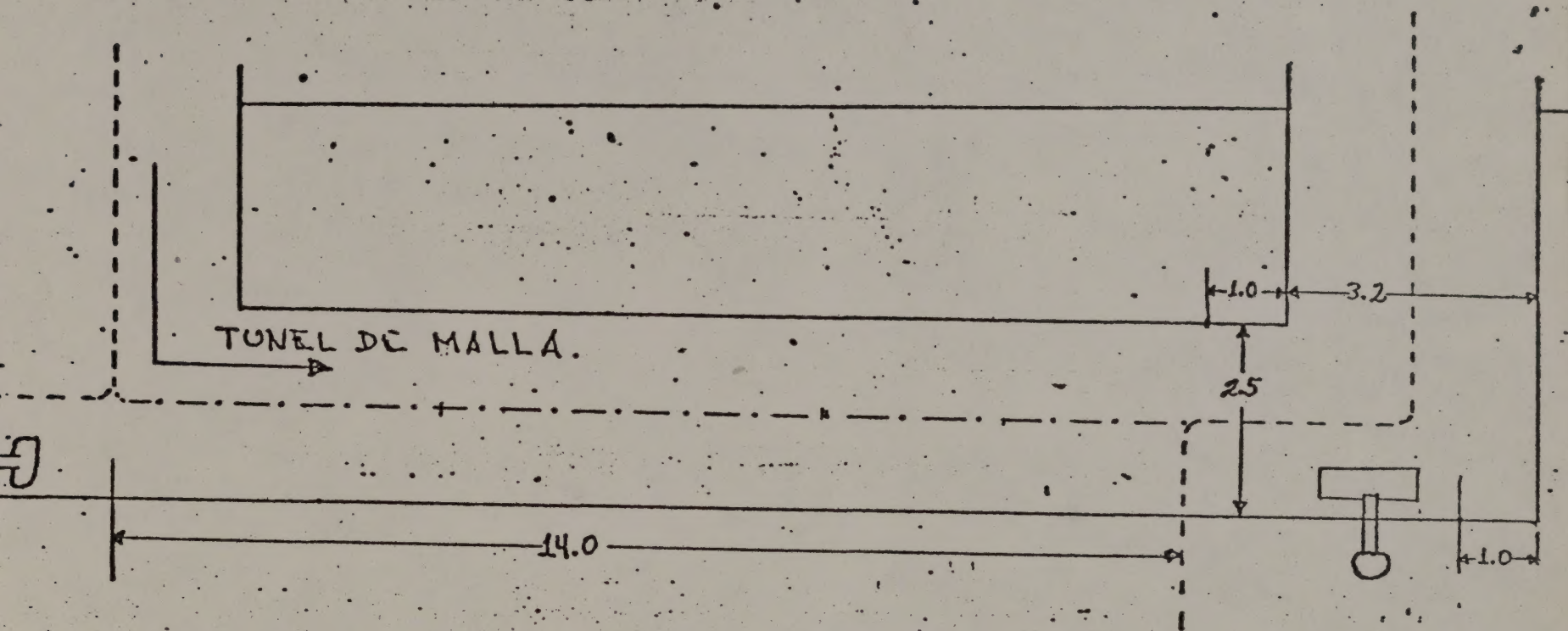


RAQUEL FCO. COURTOIS RUIZ.

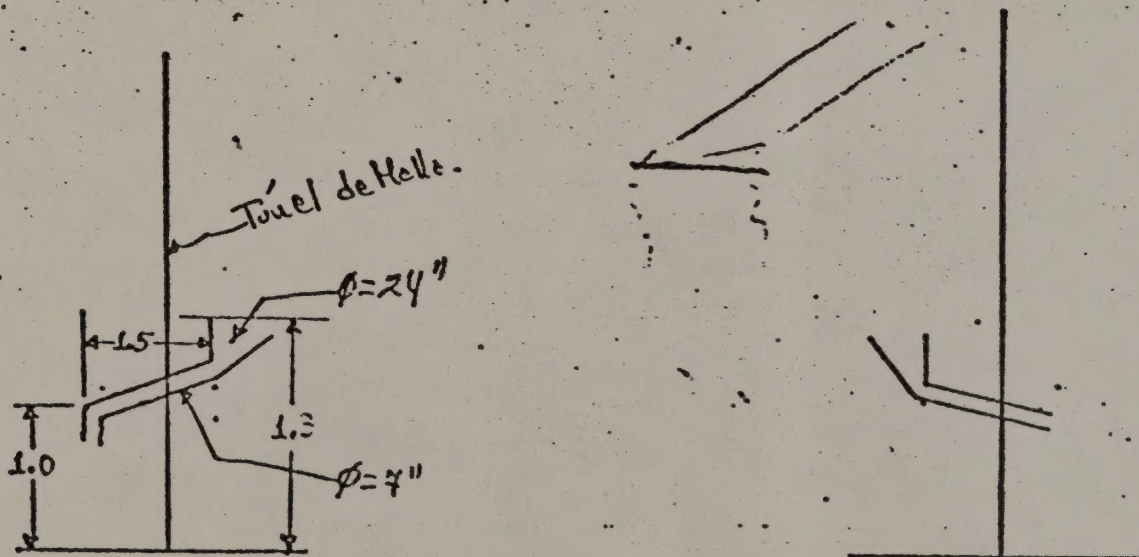
Biotéc. Superv. Pbas. de la Planta.

- c.- Subdirectores de la Planta.
- c.- Jefes de Producción.
- c.- Drs. H.C. Hoffmann y M. Vargas T.

5½ Días.



----- Honorriel existente.
 - Honorriel faltante (14.0 mts.).



Banda de Separación.

17 de Agosto de 1981.

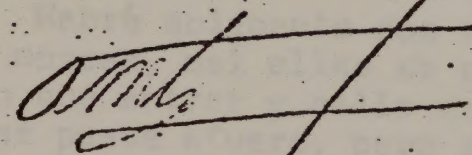
REF.: PGD*025.

DR. EDUARDO RIOS SALAS,
MR. WILLIAM H. SUDLOW
SUBDIRECTORES DE PRODUCCION,
EDIFICIO.

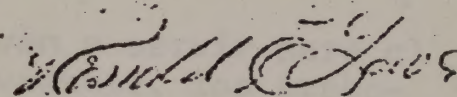
A la presente anexo un proyecto del señor Raguél Eco. Courtois R., para controlar la contaminación de mosca doméstica en el interior de la Planta.

Nosotros consideramos este proyecto muy protector y oportuno ahora que dicha infestación amenaza otra vez nuestra meta de producir 550 millones. Sugerimos que se acepte y lleve a la práctica.

Atentamente,



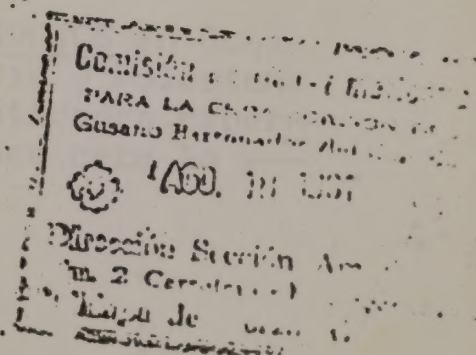
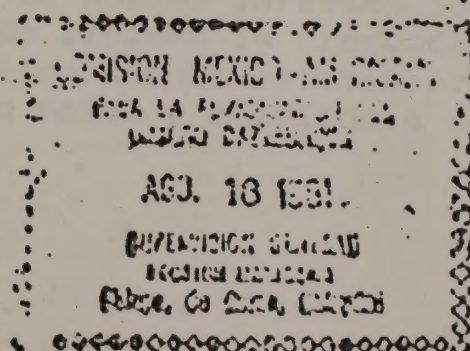
DR. ARTURO MARTINEZ Y TAPIA
Jefe del Depto. de Investigación
y Desarrollo Experimental.



DR. RONALD E. LOVE,
Ch. Experimental & Investi-
gation Department (Acting).

- c.c.p. Directorado de la Comisión.- Oficinas Centrales.
- c.c.p. Subdirectores generales.- Oficinas Centrales.
- c.c.p. Entomólogos Asesores.- Oficinas Centrales.
- c.c.p. Archivo.
- c.c.p. Consecutivo.

*alma.



COMISION MEXICO-AMERICANA PARA LA ERRADICACION DE
GUSANO BARRENADOR DEL GANADO

Document #3

LA ENERGIA ATOMICA AL SERVICIO DEL GANADERO

DIVISION DE OPERACIONES DE CAMPO
A. R. K. A. V.
CENTRO DE DISTRIBUCION
AEROPUERTO FEDERAL FRANCISCO SARABIA

APARTADO POSTAL No. 156 TELEFONO 2-35-67
BUXTELA GUTIERREZ, CHIAPAS

Hopkins

Agosto 17, 1961.

PARA: DR. ARTURO MARTINEZ Y TAPIA
DR. RONALD LOUE,
JEFES DE DESARROLLO DE METODOS.

ASUNTO: El Plan de Raquel para controlar las moscas caseras en la Planta.

Por medio de la presente comunico a Uds. lo siguiente:

Esta sugerencia de Raquel es buena. El elemento de control es mecánico puro, las moscas caseras no podrían salir del área de emergencia y ovipositar a la dieta de las larvas de gusano barrenador, y por eso no podrían devolver nuevamente a su ciclo de vida.

El método no va a depender del buen trabajo del elemento humano que, por necesidad, va a completarla. Porque las personas no completaron tareas críticas, y todo plan factible del pasado ha fallado.

Habrán solamente dos rutas posibles que van a servir para el escape de las moscas, así ellas se podrán propegar nuevamente, los trabajadores tienen que entrar y salir del área controlada y ocasionalmente van a necesitar pasas afuera, para reparación de anaqueles (que lleven chisoles de pupa) que atraviezan por el monorriel, y dichos anaqueles tendrían que pasar por puertas.

Para prevenir el escape fácil de las moscas, los trabajadores entrarían y saldrían dentro de una serie de dos corredores oscuros y pintados de negro con puertas sin aberturas, también pintados de negro.

Cuando tuviéramos que mandar anaqueles afuera los pasaríamos dentro de puertas especialmente para este propósito y abriríamos estas puertas solamente después, las moscas que pudieran escaparse estarían atraídas del área de las puertas con luces o podríamos matarlas mecánicamente.

El problema de las moscas caseras está MATANDO en Desarrollo de Métodos, miles de moscas infestan la dieta larveria y molestan a las personas; las moscas impiden la producción de las cantidades esperadas (en este período crítico cuando estamos produciendo pupas para la prueba de ARS) y bajan la moral y eficiencia del personal.

El Plan de Raquel, es digno de esfuerzos y gastos neces-
rios.

ATENTAMENTE.

D.E. Hopkins
MR. D.E. HOPKINS.

c.c.p. Mr. William H. Sudlow.
Dr. Eduardo Ríos.
Ing. Raquel Curtois.

COMISION MEXICO-AMERICANA PARA LA ERRADICACION DEL GUSANO CARRENERO
LA ENERGIA ATOMICA AL SERVICIO DEL GANADERO



KILOMETRO 2 CARRETERA A LA ANGOSTURA
MUNICIPIO DE CHIAPA DE CORZO, CHIAPAS
APARTADO POSTAL 544
TUXTLA GUTIERREZ, CHIS.

October 9, 1981

TO: MR. WILLIAM H. SUDLOW
SUB-DIRECTOR OF PRODUCTION

SUBJECT: POSSIBLE CASH AWARD RELATING TO A SUGGESTION FOR CONTROL OF
HOUSEFLIES IN THE SCREWORM PLANT

An infestation of houseflies has, generally, been present in the screwworm plant at Tuxtla Gutiérrez since the start of the production of screwworm larvae in 1974.

Housefly larvae breed and are automatically collected along with screwworm larvae and pupae in the production system. The pupae are stored, and when the screwworm pupae are 5½ days of age they are irradiated and shipped. The houseflies emerge before irradiation and remain in the plant to recycle.

On August 14, 1981, Sr. Raquel Fco. Courtois Ruiz, Biotech. Supervisor for Methods Development and I purposely went to the site of pupae storage to discuss the problem and perhaps find a solution. I expressed the thought that perhaps we could cover each group of pupae with a sack made of screen which would retain all emerged houseflies, and then fumigate and kill all of the houseflies before irradiation. Sr. Courtois then expressed the thought that perhaps we should isolate the site of pupae storage with a wall made of screen, and pass pupae in and out of the area through ducts; this would prevent all emerged houseflies from gaining access to the production area, and thus prevent recycling. I immediately considered Raquel's idea as the most feasible permanent solution that had yet been advanced by anyone after years of wrestling with the problem. I urged him to write down the idea which he did that very day, Friday August 14, (a copy of the original Spanish version and an English translation are enclosed).

When Sr. Courtois completed the written description of his idea about 16:30 hrs. we talked it over with Dr. Ronald E. Lowe, acting chief of Methods Development at Tuxtla Gutiérrez. On Monday August 17, we, Sr. Courtois and I, talked to those in authority at the plant and I submitted a letter in support of the idea (the original Spanish version and an English translation are enclosed).

Mr. Sudlow and all others who studied the idea were enthused over its possibilities and proceeded to develop it and start modifications of the storage area.

.....

The idea was Sr. Courtois's; perhaps our meeting stimulated its conception. Above all, our meeting that day did result in the development of the idea; if it works the production of screwworm pupae will surely be enhanced and a severe nuisance will be eliminated; it will be a major contribution to the program.

It is hoped that credit will be given where it is possible and where it is deserved.

Donald E. Hopkins
DON E. HOPKINS

c.c. Mr. Raquel Fco. Courtois

*svrg

U.S. DEPARTMENT OF AGRICULTURE

SPEED MEMO

PART NUMBER

DATE

3

October 15, 1981

SUBJECT

CONTROL OF HOUSEFLIES AT THE
SCREWORM PLANT

TO

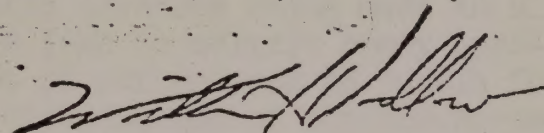
DR. M. E. MEADOWS

FROM

MR. WILLIAM H. SUDLOW

MESSAGE

I recommend that some type of incentive award be given for this suggestion.



MR. WILLIAM H. SUDLOW

SIGNATURE

REPLY

SIGNATURE

DATE

A: ALL U. S. PERSONNEL - TUXTLA

DE: DR. RONALD E. LOWE

MEMORANDUM

FECHA: SEPT. 24/82

REFERENCIA:

MODIFICATIONS OF EXISTING
SYSTEMS, TECHNIQUES, AND METHODS
FOR MASS-REARING SCREWORMS

For any large, on-going company, organization, or program (whether service or commercial, private or governmental) that wishes to remain competitive and succeed in their goal, an effort must be made to seek new and improved methodology. Considering the rapid scientific advances being made in all technical areas at the present time, I would like to solicit every possible new idea that could lead to improvement of our program in Tuxtla for mass-rearing, handling, transportation and distribution of sterile screw-worm flies, with the goal of eradicating this pest from Mexico.

I am well aware that many ideas for improvement (as ridiculous as they sound at the moment) are brought up in discussions at lunch, coffee breaks, social gatherings, staff meetings, and "bitch sessions". Many are also discussed seriously and somehow lost in day-to-day priorities, until a later date. The purpose of this memorandum is to request that all ideas, however ridiculous they seem, be presented for consideration. I would like for your mind to be put into a "Think Tank", and let it roam free into any area in which you have interest. I want you to come up with suggestions that would benefit the program financially, in efficiency, in labor, in quality or quantity of the product, or any combination thereof.

We, at the present, seem to be "locked-in" to methods that have been shown to be sufficient in the past. I would like to think that there are always new developments occurring that could improve our systems, and therefore increase our efficiency. As an example, let me give you a few of my ideas. When I came to Tuxtla, we were still using a "box" eggging board that required extensive carpentry, dowels and fasteners, and that was easily damaged during daily use. With cooperation of the BioTechs, we tested cut PVC pipe, angle iron, etc., and found that simple 2" x 2" boards could be soaked and used as an oviposition site. This has now been used for two years with efficiency of "construction", collection, and no repair. This was not an idea that recommends merit, but it works --and that is the basis of the program-- to make things happen!

Let me give you a few other things to consider! Why egg flies only four hours on our schedule? Why not collect all eggs from all females up to total first oviposition? Why not have a continual oviposition system --be it either in cages or in an open room? I can envision a large room with a specific quantity of pupae deposited each shift (day?), and specific oviposition sites from which eggs are collected on a time basis. Perhaps why not put oviposition media in each cage each shift and remove it every eight hours for a specific time, to be assured that the optimum number of eggs are collected?

Diet studies should be given a high priority for both larvae and adults. Do we need honey in the cages since we add some to the meat? Do we even need

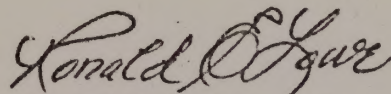
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the meat? Are the paper-strip resting areas necessary or does the actual size make a difference? Going to a gel-type larval diet would alleviate problems with vacuum, numerous feedings, quantification, labor and waste disposal. The use of acetate is an interim step but to salvage all production a means to drive off the last larvae may involve adding another material such as alcohol, ammonia, vinegar, a powder, etc. Could we go to "naked pupation" to do away with the use of sawdust? Is there a way to store extra eggs for longer periods, such as freezing, drying, nitrogen, CO₂, etc? Other than genetic sexing techniques, can we achieve sexual separation by use of floatation, centrifugation, air currents, weights, or density? Can phototropism or the diurnal photoperiod be used to advantage in any other stage? Are there any simple techniques to salvage more insects at each step of handling? Considering results of recent pupal transport tests, what actually are the limits and tolerances of time, temperature, air exchange and maturity during transport? These are perhaps only a few of the ideas to be considered.

With the understanding that administrative and financial support for a "Research and Development" area in Tuxtla has been sadly lacking in the past, but with the knowledge that the need is now recognized and being rectified, I solicit your ideas for consideration. None are too big or too small - too quick or too long-range - too cheap or too expensive. There need be no justification for an idea - only a reason and a "gut-feeling" - to include it in your suggestions. I am sure that many of us will duplicate ideas of others, which only enforces the fact that there is a problem in a specific area and effort must be put into resolving the existing problems.

Obviously, we are not a research organization, but it may be possible for some ideas to be used immediately. Naturally, suggestions will be considered on the basis of time, space, personnel, benefits, cost, and a realistic approach. I would appreciate it if you would submit written ideas and suggestions to me by October 1st, so that plans can be made for the new fiscal year. Individual interest, providing new ideas that are accepted in a team effort, will benefit the program.

Sincerely


DR. RONALD E. LOWE
SUB-DIRECTOR OF PRODUCTION

c.c. Dr. Norvan L. Meyer
c.c. Dr. Robert E. Reichard
c.c. Dr. James E. Novy
c.c. Dr. Chris H. Hofmann
c.c. Mr. William H. Sudlow
c.c. Dr. Hugh O. Graham
c.c. Dr. John P. Spencer



United States
Department of
Agriculture

Agricultural
Research
Service

Southern Region
Oklahoma-Texas Area

046
F & B Road
P.O. Drawer EC
College Station, Texas
77841

September 27, 1982

SUBJECT: Justification for Term Appointment

TO: Ella Brown

The following is a justification for the term appointment of a Research Associate Position (Research Geneticist) at Tuxtla-Gutierrez, Chiapas, Mexico:

"The genetic aspects of the research concerned with breeding structures of native screwworm populations at Tuxtla-Gutierrez, Chiapas, Mexico, are expected to be completed in 2 years. Furthermore, standard employment (contracts) is set at 2 years in Tuxtla-Gutierrez."

R. A. Hoffman

R. A. HOFFMAN
Associate Area Director

cc:

→ O. H. Graham



COMISION MEXICO-AMERICANA PARA LA ERRADICACION DEL GUSANO BARRENADOR

LA ENERGIA ATOMICA AL SERVICIO DEL GANADERO

COMTA. 24363

KM. 2 CARRETERA A LA ANGOSTURA
CHIAPA DE CORZO, CHIS.

APDO. POSTAL 544

September 27, 1982

TO: DR. RONALD E. LOWE
SUB-DIRECTOR OF PRODUCTION

SUBJECT: WEEKLY ACTIVITY REPORT

(Tests concerning production of larvae)

Typically, screwworm larvae developing in a wound collect in pockets and continually imbed, withdraw, and reimbed their mouthhooks in tissue. The larvae feed principally on body fluids and use their mouthhooks to anchor themselves within the wound and to erode tissue to make room for their increasing size.

The food supply for the larvae is continually renewed as body fluids flow to the wound. The constant drainage of fluid carries away waste products of the digestive system of the larvae and dislodged and decomposing tissue.

To rear larvae artificially a process that mimics the natural process would be desirable and a solid impermeable material heavier than water with a texture and particle-size that would accomodate the feeding habits and not filter the solids in the liquified diet (a material such as granules of nylon or silicon) might serve as a substrate for developing larvae and would possess some desirable qualities that cotton linters (or a gelled-diet) does not; some of those qualities are listed below:

1. A constant or periodic drainage and renewal of fluid could be easily engineered without vacuuming because the only waste product would be fluid.
2. If an exchange of fluid were ample practically all of the surface area of a vat could be utilized by feeding larvae (a feature not practical with a gelled-diet). Therefore, production related to unit of surface area would be most efficient.
3. Harvest of all larvae would be possible because no solid material would be discharged. Therefore, the selection of a strain that had only a certain range of time to reach full growth would not occur and cost-efficiency related to number of larvae fed to number utilized would be greatest.

A small-scale test was conducted in which aquarium gravel was used as a permanent substrate. New third instar larvae were placed in a plastic tray that contained aquarium gravel, at a depth of about 1.5 cm, that was flooded with the standard liquid diet. On day-1 the larvae were introduced. On day-2 the old diet was replaced by new diet. On day-3 new diet was added to compensate for evaporation. On day-4 most of the larvae had completed feeding and had crawled-off to pupate in the sawdust provided. Approximately 250 pupae were harvested from two trays about 10 x 20 cm.

Aquarium gravel was probably the least acceptable material that could be used but none other was readily available to run this very preliminary test. However, the results did indicate that an acceptable system might be developed.

Sincerely

Donald E. Hopkins
DONALD E. HOPKINS
SUPVRY. BIOTECHNICIAN

c.c. Dr. Arturo Martinez y Tapia
c.c. Dr. H. C. Hofmann
c.c. Dr. Moises Vargas Terán
c.c. Dr. O. H. Graham
c.c. Sr. Raquel Courtois
c.c. Sr. Charles McVean
c.c. Dr. John P. Spencer

*svrg



RECEIVED OCT 13 1982

COMISION MEXICO-AMERICANA PARA LA ERRADICACION DEL GUSANO BARRENADOR

LA ENERGIA ATOMICA AL SERVICIO DEL GANADERO

Septiembre 28, 1982

DR. NAZARIO PINEDA VARGAS
DR. ROBERT E. REICHARD
DIRECTORES.

Por éste conducto nos permitimos solicitar su aprobación para la ejecución del protocolo adjunto, encaminado a la reevaluación del sistema "SWASS" en el Valle de Tierra Caliente, del estado de Guerrero.

Atentamente.

DR. MOISES VARGAS TERAN
Entomólogo Asesor.

Vo.Bo.

DR. NAZARIO PINEDA VARGAS
Director

c.c.p. Dr. Mario A. Licon
Dr. Agustín Pagaza
Dr. Enrique Asín
Biol. Raquel Orozco
Dr. Arturo Martínez y T.
Dr. Humberto Padilla
Sr. Wilford Fansworth
Sr. George Moud
Dr. H. Graham
Dr. John Spencer

MVT/lrr

DR. H. C. HOFMANN
Assistant Entomologist.

Vo.Bo.

DR. ROBERT E. REICHARD
Co-Director

Dr. James Novy
Dr. Edgard L. Judy
Dr. Enrique Espinoza
Sr. Jim Bruce
Dr. David Hoffman
Dr. Fernando Perez C.
Dr. David W. Anderson
Cap. Walter Jensen

Protocol for
SWASS test
conducted at
Altamirano
was attached -
see SWASS Test
folder for
a copy



University of Illinois at Chicago

College of Liberal Arts and Sciences
DEPARTMENT OF BIOLOGICAL SCIENCES
Post Office Box 4348
Chicago, Illinois 60680
(312) 996-2211

UNIVERSITY CENTER

September 28, 1982

Dr. O.H. Graham
USDA
Screwworm Research
P.O. Box 986
Mission, Texas 78572

Dear Hugh:

I have enclosed a copy of our hominivorax paper for you to have. This is the work Don Baumgartner and I did in Peru in the context of our research on blow fly biology during the last few years. The field work is now completed (more accurately, the field work is not finished, just the funding) and write-ups on life cycles, Chrysomya synanthropy and New World distributions, and the ecology of about 20 calliphorid species are under way. Some of this will be presented at the Toronto meetings. In the meantime, I wanted to take this opportunity to express my gratitude for your interest and help. We would appreciate any comments you and your colleagues may have on the paper.

Sincerely,

A handwritten signature in blue ink that reads "Bernie".

Bernard Greenberg
Professor

BG:rp

Copy of mes. in SCREWORM reprint file



United States
Department of
Agriculture

Agricultural
Research
Service

Southern Region
Oklahoma-Texas Area

Screwworm Research
P. O. Box 986
Mission, TX 78572

September 30, 1982

Dr. David B. Taylor
Department of Biology
University of Notre Dame
Notre Dame, Indiana 46556

Dear Dr. Taylor:

Dr. R. L. Mangan handed your letter of September 14 and the enclosed curriculum vitae to me for reply. We appreciate your interest in the insect geneticist position in our laboratory and will keep you informed about developments. I anticipate that the ARS Personnel Branch, Special Examining Unit, in Hyattsville, MD, will announce the vacancy next month and we hope that you will apply. You already know, I believe, that our laboratory is located in Tuxtla Gutierrez, Chiapas, Mexico and that scientists who transfer to that location at USDA's expense, are obligated to remain for two years. Primarily the research will be concerned with breeding structures of native screwworm populations and the quality and competitiveness of mass-reared, sterilized screwworms. If you have other questions I would be happy to try to answer them.

Sincerely,

O. H. GRAHAM
Location/Project Leader

cc:

R. A. Hoffman
R. A. Bram
R. L. Mangan

SOUTHWESTERN ENTOMOLOGICAL SOCIETY

P. O. Drawer DG
College Station, Texas 77841

September 30, 1982

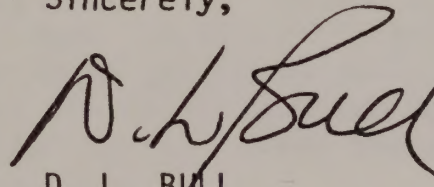
O. H. Graham
P. O. Box 986
Mission, TX 78572

Dear Dr. Graham:

Enclosed herewith is manuscript SWE# 442 entitled "Screwworm Eradication Program--Procedural Update of the Mission, Texas Mass-Rearing Program from 1966 to 1980" which has been submitted to the SOUTHWESTERN ENTOMOLOGIST for consideration for publication. It would be greatly appreciated if you could review this manuscript for this journal. If you are unable to do this within three weeks of receipt, please return the manuscript to me. In this case it would be most helpful if you would recommend one or more reviewers.

A review form for your comments on the manuscript is enclosed.

Sincerely,


D. L. BULL
Editor

DLB/11w

Enclosures

*returned to
Don Bull
10-15-82*



United States
Department of
Agriculture

Agricultural
Research
Service

Southern Region
Oklahoma-Texas Area

Screwworm Research
P. O. Box 986
Mission, TX 78572

October 1, 1982

SUBJECT: Improvements in the Methodology for Screwworm
Rearing

TO: Dr. Ronald E. Lowe
USDA-APHIS-Screwworm Eradication
Tuxtla Gutierrez, Chiapas, Mexico

Thank you for including ARS in the distribution of your memorandum dated September 24; I congratulate you on having clearly identified some of the most urgent needs of the program in the area of improved methodology. Furthermore, most, perhaps all, of the problems are solvable and their solution will significantly expedite establishment of the permanent barrier in Tehuantepec. Also, your memo is very timely. I am deeply impressed with the professional qualifications and attitudes of the newly arrived entomologists and feel that there is real hope for some important breakthroughs in the near future.

While it is difficult, probably impossible, to unmistakably distinguish between ARS and Methods Development responsibilities and activities I have no worries about possible overlap or duplication of effort and even less concern about competition between the two groups. There are so many things to do and communication between the two groups is so good that each individual can have an important project and be sure that his or her individual accomplishments will receive proper recognition. I am eager to hear about the suggestions and recommendations that are being submitted to you.

At this time I will make only a few very specific suggestions but after I am stationed in Tuxtla I will probably be approaching you almost every day with new ideas. For the moment, please consider these:

1. Replacement of cotton linters with a more efficient substrate or larval medium. Several people in Methods have some very good ideas. Also, Dr. R. L. Harris at College Station has already prepared some simple gels which appear to be good and might be exactly what you need. The next step will be to determine whether or not screwworms thrive on one or more of these gels. If a gel meets all of the biological requirements, the engineering aspects will have to be studied - some re-engineering will certainly be required.
2. Heat the vats on the main rearing floor. To me this is fundamental; I do not believe that the quality of the flies will ever be satisfactory when Groups 1 and 2 are reared on cold vats. I fully appreciate that it will not be easy or cheap to reconstruct vats etc., but I firmly believe that the change will more than repay its cost in the form of an improved sterile fly that will get the program moving south again.
3. Larval-pupal separator. It has not been operating efficiently. I understand that the old belts will soon be replaced with new ones and this

should help but in addition a system should be devised to make it impossible to overload the belts. If the machine is to operate efficiently the belts should not be covered with pupae; if at all possible not more than the center one-third of the belt should be loaded with pupae.

O. H. Graham

O. H. GRAHAM

Location/Project Leader

cc:

Norvan Meyer

R. A. Bram

R. A. Hoffman

R. E. Reichard

J. E. Novy

H. C. Hofmann

W. H. Sudlow

J. P. Spencer

H. E. Brown

C. J. Whitten

R. C. Bushland

R. L. Harris

D. E. Hopkins

O. H. Graham

O. H. GRAHAM

Location/Project Leader

*see INCENTIVE file
for enclosure*



United States
Department of
Agriculture

Agricultural
Research
Service

Southern Region
Oklahoma-Texas Area

O. H. Graham
Screwworm Research
P. O. Box 986
Mission, TX 78572

October 1, 1982

Dr. R. A. Roncalli
Animal Science Research
Merck Sharp & Dohme Research
Laboratories
P. O. Box 2000
Rahivay, New Jersey 07065

Dear Dr. Roncalli:

Your letter of September 22 and the sample of IVOMEC Injectable both arrived here on September 27 and I was able to forward the sample to our laboratory in Tuxtla Gutierrez on the same day. We very much appreciate both the sample and the information you sent.

The Statement of Investigator and Notice of Trial have been completed and are enclosed. With best personal regards, I am

Sincerely,

O. H. Graham

O. H. GRAHAM
Location/Project Leader

*see IVERMECTIN file
for enclosures*



United States
Department of
Agriculture

Agricultural
Research
Service

Southern Region
Oklahoma-Texas Area

Mr. Graham
Screwworm Research
P. O. Box 986
Mission, TX 78572

October 1, 1982

SUBJECT: Controlled Release of Swormlure

TO: Dr. H. E. Brown
Dr. J. W. Mackley

This week I have talked by telephone first to Dr. Morton Beroza, an internationally known attractants chemist, and then to Dr. Agis Kydonieus of Healthchem Corporation in New York. Both feel that it should be possible to devise a controlled release system by which swormlure would emanate from a SWASS pellet or other device at any desired rate. I am sending some information about Swormlure and SWASS to Dr. Kydonieus and will discuss some of his ideas with him at a later date. If either of you would like to discuss any aspect of the matter with him he can be contacted at the following address:

Dr. Agis Kydonieus
Healthchem Corporation
1107 Broadway
New York, N.Y. 10010
Tel. 212/691-6417

O. H. Graham
O. H. GRAHAM
Location/Project Leader

cc:

R. A. Bram
R. A. Hoffman
Jack Plimmer

U.S. DEPARTMENT OF AGRICULTURE

DATE

REFERENCE SLIP

10-6-82

TO

Braham

- | | |
|---|---|
| ☐ ACTION | ☐ NOTE AND RETURN |
| ☐ APPROVAL | ☐ PER PHONE CALL |
| ☐ AS REQUESTED | ☐ RECOMMENDATION |
| ☐ FOR COMMENT | ☐ REPLY FOR SIGNATURE OF |
| ☒ FOR INFORMATION | ☐ RETURNED |
| ☐ INITIALS | ☐ SEE ME |
| ☐ NOTE AND FILE | ☐ YOUR SIGNATURE |

REMARKS

FROM

P. A. Braham

OX-LIKE TRAP MAY STAMP OUT SLEEPING SICKNESS

Harare THE FINANCIAL GAZETTE in English 6 Aug 82 p 8

[Text]

A REVOLUTIONARY tsetse fly trap which could eradicate sleeping sickness — trypanosomiasis — in Africa is now being tested on an island in the middle of Lake Kariba.

The trap, which is undergoing research at the Tropical Products Institute in London, would be cheaper and more ecologically sound than present methods.

The tsetse fly inhabits 15 million square kilometres of central Africa and countries affected include Zimbabwe, Tanzania, Botswana, Zambia, Kenya and Uganda. Methods of wiping out the insect have concentrated on spraying — killing the ground-based tsetse fly as it comes up to feed — or wiping out the host animal. Both methods have disadvantages.

Dr David Hall, a TPI chemist working on the new trap, explained: "The destruction of game animals is not popular and the use of insecticides is coming under increasing criticism on economic and ecological grounds.

EXPENSIVE

"With the trebling of wages in Zimbabwe spraying is becoming expensive and an affected area can only be sprayed at certain times of the year: in the rainy season the insecticide is washed away."

Tsetse flies rely primarily on vision and odour and the trap, pioneered by Dr Glyn Vale of the

Department of Veterinary Services, Harare, Zimbabwe, catches the flies simply by looking and smelling like an ox. Once inside, the flies are killed or sterilised.

Dr Vale found that massive doses of carbon dioxide and acetone supplied part of this smell and made the large black visual traps far more enticing to tsetse flies, but it was still not as good as the real thing.

ATTRACTIVE

Work on identifying specific chemicals attractive to tsetse flies is being carried out by the TPI team of Dr Brenda Nesbitt, Mr Peter Beevor and Dr Hall. Methods used by the department in its work on insect pheromones (sex attractants) have been used in their present work. The flies smell through receptors in their antennae and, by measuring the electrical changes in the antennal response when exposed to a particular odour, the chemists are able to identify which chemical is most attractive to the insect.

"We've had some positive results already," Dr Hall said. "We've identified and synthesised one chemical and are getting good results from it. By this method there is a definite possibility of eradicating the tsetse fly, particularly as they breed at a low level.

"All the indications are that the system works. Whether it will revolutionise tsetse fly control is another matter, but it is an important element in their control."

The TPI team hopes to finish its research within a year.

CSO: 5400/5725



United States
Department of
Agriculture

Agricultural
Research
Service

Southern Region
Veterinary Toxicology
and Entomology
Research Laboratory

P.O. Drawer GE
College Station, Texas
77841

RECEIVED OCT 15 1982

October 8, 1982

SUBJECT: Foreign Travel Report

TO: E. L. Kendrick
Regional Administrator
USDA, ARS, SR
P.O. Box 53326
New Orleans, LA 70153

Attached are eight copies of my Foreign Travel Report as requested.

R. L. HARRIS
Research Leader
Biological Control Research Unit

Enclosure

cc: w/encl.

J. R. Johnston, AD

D. A. Witzel, LD

H. Graham, RL

R. A. Bram, NPS

R. O. Drummond, LD

United States Department of Agriculture, Agricultural Research Service,
Southern Region -- FOREIGN TRAVEL REPORT

1. Traveler; title; laboratory; center or field station affiliation; location; and NRP:

R. L. Harris, Research Leader
Biological Control Research Unit
Veterinary Toxicology & Entomology Research Laboratory
College Station, Texas 77841
NRP - 20480

2. Country or countries visited and period of travel:

Mexico
September 20, - September 27, 1982

3. Purpose of Trip: To discuss the possibility of developing a new screwworm larval rearing media and to observe the rearing procedure at Tuxtla Gutierrez, Chiapas, Mexico.

4. Summary: The screwworm rearing facility at Tuxtla Gutierrez, Mexico, is presently producing over 400 million screwworms per week. The principle problems is finding a substrate for the liquid larval media. A cotton linter is presently being used, but disposal, loss of larvae (10-20%), and spent media removal (vacuuming) are major drawbacks. The use of a gel media may solve many of these problems. The production personnel are extremely interested in developing a better rearing media. Adequate facility and equipment are available at both ARS and APHIS laboratories to conduct rearing experiments.

5. Travel Details: The first discussion was with Drs. Lloyd Wendell and Harold Brown. Dr. Wendell indicated that a limited amount of research has been conducted on a gel media for rearing screwworm larvae. The larvae have been reared successfully on an agar-based media. Dr. Wendell has already developed a gelled adult diet, while Dr. Brown has contacted a New Zealand dairy products company about obtaining a casein-based gel. The company has agreed to formulate a gel using the basic screwworm rearing nutrients.

I attended a technical conference in Mexico City, Mexico, where we discussed technical problems. Many of the screwworm eradication officials attended this conference and discussed, in general terms, the problems with screwworm rearing. They indicated that the use of cotton linters was one of the biggest problems in screwworm rearing.

We discussed with Dr. Ron Lowe, Production Manager, Screwworm Production Plant, Tuxtla Gutierrez, Mexico, various production problems. He again said that cotton linters in the diet was a tremendous problem. The incinerator and expeller were broken and the media disposal was a problem. Many larvae are lost (10 to 20%) when the linters are removed. They have located a source of cellulose acetate and will test it as a replacement for cotton linters. This should solve some of the rearing problems, however a gel media would be more satisfactory. They are presently producing over 400 million flies per week and a gel media should aid to increase production.

Dr. Chuck MacVean conducted a tour through the rearing plant and explained the rearing procedures. The mechanical problems with liquid media on cotton linter appears to be the principle problem and the methods development area is tied up with strain change. Methods development area and equipment need to be repaired and modified before it can begin additional tests. Methods development personnel are very interested in working on the problem of liquid media and will conduct tests when space and equipment are available.

We discussed production problems with Dr. Ed Gersabeck. He explained some of the procedures and we discussed the need for heating the vats. The screwworm larvae and bacterial growth are probably providing heat at this time, but if a gel media is developed and used, the vats will need to be heated.

I toured the ARS research facility and briefly discussed with each scientist their research. They have a wide research program, however research on artificial rearing is not being conducted at this time. There are adequate facilities and equipment to conduct rearing research.



United States
Department of
Agriculture

Animal and
Plant Health
Inspection Service

Veterinary
Services

National Veterinary Services Laboratories
P. O. Box 844
Ames, Iowa 50010

Subject: Fly Survey in Texas

RECEIVED OCT 25 1982

To: D. R. Cassidy, Chief
Pathobiology Laboratory

Date: October 12, 1982

Through: R. K. Strickland, Head
General Pathology and
Parasitology Section
Pathobiology Laboratory

Copies to: Dr. Brown
Dr. Hugh Graham
Dr. Reichard
Dr. Chris Heffernan
Dr. James Whiffen
Mr. Bill Sadlow

On September 21, 1982, a survey was initiated in central Texas, as part of an ongoing surveillance for the blowfly Chrysomya rufifacies. Mr. Elmer Ahrens, Regional Entomologist, USDA, APHIS, Kerrville, Texas, and myself set a series of swarm lure baited wind oriented traps, north and east of Kerrville, Texas. These traps were provided by the screwworm program, Mission, Texas. The swarm lure used in this test was the #4 formulation.

Over the 8-day period of trapping 282 Chrysomya rufifacies adults were trapped, of which 83% were females. The Chrysomya responded well to the new volatile formulation of swarm lure, with all the traps in the 280 mile test area catching Chrysomya rufifacies. All traps caught C. rufifacies within 48 hours after set up, and trap catches ranged from 2 to 30 adults caught within a 48 hour period. The survey area extended from Kerrville, Texas, to a point about 40 miles south of the Oklahoma border, near the town of Graford, Texas. Generally speaking the weather conditions for the survey area were dry, with daily temperature fluctuation between 50° F to 94° F. Traps were placed in areas with diverse environmental conditions (dry, wet, near livestock, not near livestock) all traps caught Chrysomya.

The site of Kerrville, Texas, was chosen for the initial point for this survey because it is 80 miles north and east of the previously known northern distribution for C. rufifacies. From the data collected it is obvious that this recently introduced blowfly is firmly established over large areas of Texas. With the widespread distribution and high population density of C. rufifacies in Texas, it seems apparent that the fly is not exhibiting a preference for living tissues, as had been reported in Hawaii (Shishido and Hardy). If these flies were involved with feeding in open wounds of livestock, it would be reasonable to assume that many larval submissions would have been sent as screwworm suspects to Mission, or to NVSL, Ames. Prior to this survey, there have been only 3 confirmed cases involving Chrysomya larvae in the US. Two have been larval submissions from dead tissue, October 3, 1981, and November 5, 1981. Most recently a larval submission was sent to NVSL, Ames, the larvae having been removed from the ear canal of a living dog in McAllen, Texas. This

D. R. Cassidy

2.

September 8, 1982, case represents the first case of myiasis involving Chrysomya larvae in the US.

Apparently this blowfly is highly competitive and has rapidly spread across Texas. The fly seems to prefer necrotic tissue as its food source, but with the demonstrated ability to invade living tissue its inevitable spread to other areas in the US should be actively monitored.

R D Richard

R. D. Richard, Entomologist
General Pathology and
Parasitology Section
Pathobiology Laboratory

cc:

A. Strating, NVSL, Ames, IA
E. Ahrens, Kerrville, TX
N. L. Meyer, VS/IP/ADA, Washington, DC
R. Morgan, VS/SCR/RD, Ft. Worth, TX
✓ F. E. Smith, VS/NPPS/SGEE, Hyattsville, MD



United States
Department of
Agriculture

Agricultural,
Research
Service

Southern Region
Oklahoma-Texas Area

Screwworm Research
P. O. Box 986
Mission, TX 78572

Dr. Graham's File

October 13, 1982

SUBJECT: Manuscript, "Body Sizes and Field Dynamics of Ground
and Aerially Released Sterile Screwworm Flies

TO: Dr. D. O. Mcinnis
Research Geneticist
Tropical Fruit and Vegetable
Research Lab
P. O. Box 2280
Honolulu, Hawaii 96804

You will note that two peer reviewers have concluded that this manuscript is not acceptable for publication and that the third, Dr. R. D. Peterson, had serious reservations about the conclusiveness of the findings. Although I encouraged you to undertake the study, I share the reviewers' doubts and have concluded that you should not attempt to publish this paper. The measurements, as you point out in the paper, were strongly influenced by extent of feeding and environmental conditions and did not reflect true size of the adult at time of eclosion. You should have used the measurement technique described by Alley and Hightower (1966) or some other variation of the technique used by Bursell (1960) and thus avoided measurement of expansible body areas.

In addition I feel that the data related to dispersion are weak in that (1) the number of trapped flies listed in Table 3 is too small, (2) the trap distribution was biased against the detection of fly movement into the prevailing wind, and (3) actual records of wind direction and velocity are not presented.

It is my conclusion that this study should be considered preliminary and that Mr. MacVean, either alone or in collaboration with others, should continue the study. Everyone suspects that the largest flies from a particular day's production live longer and move farther than the small flies from the same production but we still lack factual proof of a correlation between size and vigor.

Hugh

O. H. GRAHAM
Location/Project Leader

Enclosure

cc:
C. M. MacVean w/encl.
R. A. Hoffman
H. D. Petersen
W. H. Sudlow
Tuxtla

Separation Sheet

Collection Name: Southwestern United States and Mexico Collection:
Screwworm Eradication Program Records

Item: Dispensal data and map, 1982

Original location: Box 8, folder 113 – September – October

Subject/Series: Correspondence / Screwworm Eradication
Program

New location: Oversize, Box 103, folder 7 – September – October



COMISION MEXICO-AMERICANA PARA LA ERRADIGACION
DEL GUSANO BARRENADOR

LA ENERGIA ATOMICA AL SERVICIO DEL GANADERO

AEROPUERTO INTERNACIONAL MIGUEL HIDALGO

GUADALAJARA, JAL.

AREA III

APDO. POSTAL 1-3964

TEL. 35-86-36

RECEIVED OCT 22 1982

Octubre 14, 1982

PARA: DR. ENRIQUE ASIN A.
JEFE REGIONAL AREA III

ASUNTO: DATOS SOBRE EL AREA DE PRUEBA DE SWASS EN EL VALLE DE
TIERRA CALIENTE, ESTADO DE GUERRERO Y MICHOACAN

Adjunto al presente sírvasse encontrar mapa del area de dispersión arriba mencionada, así como datos de la parrilla UPS -3, misma que hace un total de 8 vuelos en Cessna 206 con 2460 Lbs. de Swass y 27.4 Hrs. de vuelo.

Agradeciendo de antemano la atención que se sirva prestar -
al presente quedo

Cordialmente,

SR. ERNESTO C. GUZMAN
COORDINADOR DISPERSION

c.c.p. Dr. Hoffman, Dr. Pagaza, Dr. Padilla, Dr. Espinoza, Dr.
Judy, Dr. Anderson, Dr. Moises Vargas, Mr. Farnsworth,
Cap. Jensen, Mr. Jim Bruce, Dr. H. Graham, Sr. George
Mudd.

ECG*1pg

DEPARTMENT OF JUSTICE

WASHINGTON, D. C. 20535

RECEIVED OCT 1 1981

October 1, 1981

Mr. J. Edgar Hoover
Federal Bureau of Investigation
Washington, D. C. 20535

Dear Mr. Hoover: I am writing to you in regard to the matter of the

investigation of the activities of the [redacted] in the [redacted] area.

I am enclosing for your information a copy of the report of the

Enclosure

[Handwritten signature]
Special Agent in Charge
Department of Justice

Sincerely,
[redacted]
Special Agent in Charge

100-100

Dr. Graham



United States
Department of
Agriculture

Agricultural
Research
Service

Southern Region
Oklahoma-Texas Area

Screwworm Research
P. O. Box 986
Mission, TX 78572

October 26, 1982

FOREIGN TRAVEL REPORT

1. Traveler. D. R. Ring, Research Entomologist, TAES, Screwworm Research, Mission, Texas, NRP 20408.
2. Countries Visited. Tuxtla Gutierrez, Chiapas, Mexico.
3. Purpose of Trip. To attend research planning meeting, conduct research on competitive mating of fertile and sterile screwworms by sex and evaluate ivermectin against the screwworm in vitro.
4. Summary. Attended the research meetings and briefly described accomplished research and outlined planned research. Discussed competitive mating studies with Dr. Whitten and carried out such studies. This involved mixing 200 sterile flies (A82) with 20 fertile flies (10♂'s and 10♀'s). Treatments of sterile flies were 200-0, 150-50, 100-100, 50-150 and 0-200 (males: females) and 4 different strains of fertile flies were treated (V-81, A82, M83 and DAM-5) for 24 hr. All sterile ♀'s were examined for mating by dissection. All fertile ♀'s were allowed to oviposit or die by natural causes and then were examined for mating and egg stage. Results indicated that less sterility was obtained in cages without sterile males.

Eggs, 1st instar larvae and 3rd instar larvae were placed in media treated with 1 of 2 insecticides (coumaphos or ivermectin at 0.01, 0.1, 1, 10 and 100 ppm) or 1 of 2 controls (H₂O or acetone). 1 drop of each treatment was also placed on other eggs. Ivermectin killed larvae at ca. 10X less dose than coumaphos and both insecticides prevented larval eclosion at the higher dosages.

Dennis Ring

D. R. RING
Research Entomologist

cc:

Area Office
Norvan Meyer
Floyd E. Smith
R. E. Reichard
J. P. Spencer
H. C. Hofmann
R. E. Lowe

W. H. Sudlow
H. E. Brown
L. E. LaChance
J. W. Snow
C. J. Whitten
R. C. Bushland
R. A. Bram

Oct. 27

Dear Hugh,

I apologize for taking so long to review this manuscript, but as you can see on the review form I felt that it needs strengthening. I have no complaints with the hypothesis, just the paucity of supportive data. Perhaps I am mistaken, but I feel that reviewers for Environmental Ent. may give it a rough time.

The problem I have is that the hypothesis (V-81's effectiveness was reduced by the movement of native flies from outside the release zone to the area sampled by sentinel animals) is supported only by the significantly increased sterility at 2 (3?) coastal pens at the outset of the 2nd test. I know that field data is difficult to obtain and even more difficult to interpret, but I would feel better about Don's interpretation if there were other evidence to substantiate it.

An alternative interpretation of the data from the 2nd test is that the greater sterility in the coastal area was the result of a greater sterile:native fly ratio. A rank correlation of sterility and density presented in Table 1 was not significant. However, this was primarily because of the unusually high density (2.79) experienced at the coastal pens during the 3rd week.

Anyway, to strengthen the hypothesis presented I tried to find additional similar relationships concerning insulation from native fly movement and rate of sterility. Krafsur's data (Veracruz 1975) did not fit when analyzed via ranked correlation. (However, it was for a cumulative period of 11 weeks and therefore perhaps not applicable.) I have the weekly data by pen for the 1956 Florida test and it did fit at the cumulative of 5 weeks (but not at 3 or 4 weeks), but it also fit a density dependent hypothesis. I am including the data that I used to send to Don if you wish.

One thing that was very interesting, I thought, was the greater number of egg masses collected at pens 5-8 (Fla. test) during the 1st and 2nd weeks. These pens did not receive as many flies as did the other pens in the release zone and it was almost as though the sterile flies flushed the natives out! It's too bad that there was no pre-release data--but we never seem to have all the data we would like from field tests do we?

My point is that the manuscript needs strengthening if possible. I think you would agree that if the higher sterility observed at the coastal pens during the 2nd test was primarily a result of the area of influence being insulated, the program should anticipate few problems with sterility since the size of the program release area is much larger providing for a much larger buffered area.

Good luck on getting that buck opening day! We will try to send down some cold weather to improve your chances.

Jim

refers to mss
by McInnis et al
for Environ. Entomol
mss. carried to
Tuxtlá 03/03/83
to discuss
with Mackley
& Pete

Florida Test 1957

5/5-11

5/12-18

5/19-25

5/26-6/1

PEN Distance

1st wk

2nd wk

3rd wk

4th wk

	RANK LO-HI		%ST /n	RANK LO-HI		%ST /n	RANK		%ST /n	RANK		%ST /n	RANK	
5	1	(20mi)	17/17	7		5/57	6		2/45	2		7/54	4	
6	2		5/87	6		6/126	8		8/51	3		2/45	3	
7	3		2/44	4		5/21	6		13/16	4.5		0/10	1.5	
8	4		3/66	5		5/79	6		21/39	7		0/10	1.5	
10	5		0/14	2		0/4	2.5		20/10	6		27/15	8	
9a	6											50/20		
9	7		0/6	2		0/3	2.5		0/7	1		8/36	5	
11	8		30/10	8		0/4	2.5		13/16	4.5		12/34	6	
12	9	(35mi)	0/9	2		0/1	2.5		50/14	8		19/24	7	
			* Estimated											

Cumulative of 3 wks						Cumulative of 4 wks					
		# sterile	Total	%ST	RANK			# sterile	Total	%ST	RANK
5	1	7	119	5.9	4			11	173	6.4	4
6	2	15	264	5.7	3			16	309	5.2	2
7	3	4	81	4.9	2			4	91	4.4	1
8	4	14	184	7.6	6			14	194	7.2	5
10	5	2	27	7.4	5			6	42	14.3	7
9	6	0	16	0	1			3	52	5.8	3
11	7	5	30	16.7	7			9	64	14.1	6
12	8	7	24	29.2	8			11	48	22.9	8

$$t_s = .5238$$

$$t = 1.506 \quad 6 df$$

$$P = > .1 \quad ns$$

$$t_s = .666$$

$$t = 2.191$$

$$P = > .05 \quad ns$$

$$t_s \text{ for density vs sterility} = -.0476 \quad ns$$

$$t_s = .6190 \quad ns$$

Note: Pens 5, 6, 7, 8 were just outside area of full releases until 5/14

Releases began 5/1 but the numbers of flies were insufficient to cover the entire area

Cumulative of 5 wks

PEN	DISTANCE RANK	# Sterile	Total	%st	RANK										
5	1	15	218	6.9	3										
6	2	24	412	5.8	2										
7	3	5	111	4.5	1										
8	4	19	208	9.1	4										
10	5	18	104	17.3	7										
9	6	14	100	14.0	5										
11	7	11	78	14.1	6										
12	8	13	57	22.8	8										

$$t_s = .833$$

$$t = 3.693$$

$$P = < .05$$

$$t_s \text{ for Density vs Sterility} = .7857$$

$$P = < .05$$



United States
Department of
Agriculture

Agricultural
Research
Service

Southern Region

P. O. Box 986
Mission, Texas 78572

October 30, 1982

Mr. Mike Ferguson
Department of Entomology
Texas A&M University
College Station, Texas 77843

Dear Sir:

Will you please enter on the appropriate records that Dr. Dennis R. Ring, who is stationed at this location, was on annual leave on October 20, 21, and 22 (total 3 days).

Also, please note that the U. S. Department of Agriculture has recently adjusted its per diem rates for travel in Mexico down to a maximum of \$38 per day for all locations except for a few high rate areas (Mexico City, Acapulco, etc.). Thank you.

Sincerely,

O. H. Graham
Location/Project Leader
Screwworm Research

cc:R. A. Hoffman
D. R. Ring

